

starlings. I have also found two young birds of the *Molothrus* nearly fully feathered in the nest of a starling; but in this instance the young starlings had been ejected from the nest." He then states that he had long kept in confinement a male and female of this species of *Molothrus*, which are now six years old. The hen began to lay at the age of two years, and has laid each time six eggs, which is the number laid by *Icterus*, a near ally of *Molothrus*. The dates on which the eggs were laid this year are as follows:—February 1, 6, 11, 16, 21, and 26; so that there was an interval of exactly four clear days between the laying of each egg. Later in the season she laid six additional eggs, but at much longer intervals and irregularly, viz. on March 8, April 6 and 13, May 1, 16, and 21. These interesting facts, observed by Mr. Nation in relation to a bird so widely distinct from the cuckoo as is the *Molothrus*, strongly support the conclusion that there is some close connection between parasitism and the laying of eggs at considerable intervals of time. Mr. Nation adds that in the genus *Molothrus*, out of every three young birds he has invariably found two to be males; whereas with *Sturnella*, which lays only three eggs, two of the young birds are, without any exception, females.

CHARLES DARWIN

Down, Beckenham, Kent, November 7

The Velocity of Light

IN reply to Mr. Macaulay (*NATURE*, vol. xxiv. p. 556) I will endeavour to explain more clearly the statements, made in my former communication on this subject (*NATURE*, vol. xxiv. p. 382). On one important point the explanation will include a correction.

With reference to the group-velocity U , we know from Fourier's theorem that any disturbance travelling in one dimension, can be regarded as resulting from the superposition of infinite trains of waves of the harmonic type, and of various amplitudes and wave-lengths. And we know that any one of these trains, of wave-length λ , is propagated unchanged with a velocity V , which we regard as a known function of λ , dependent upon the nature of the medium.

Unless we can deal with phases, a simple train of waves presents no mark by which its parts can be identified. The introduction of such a mark necessarily involves a departure from the original simplicity of a single train, and we have to consider how in accordance with Fourier's theorem the new state of things is to be represented. The only case in which we can expect a simple result is when the mark is of such a character that it leaves a considerable number of consecutive waves still sensibly of the given harmonic type, though the wave-length and amplitude may vary within moderate limits at points whose distance amounts to a very large multiple of λ . We will therefore suppose that the complete expression by Fourier's series involves only wave-lengths which differ but little from one another, and accordingly write it—

$$a_1 \cos \{ (n + \delta n_1) t - (\kappa + \delta \kappa_1) x + \epsilon_1 \} \\ + a_2 \cos \{ (n + \delta n_2) t - (\kappa + \delta \kappa_2) x + \epsilon_2 \} + \dots,$$

or in the equivalent form—

$$\cos (n t - \kappa x) \sum a_1 \cos (\delta n_1 t - \delta \kappa_1 x + \epsilon_1) \\ - \sin (n t - \kappa x) \sum a_1 \sin (\delta n_1 t - \delta \kappa_1 x + \epsilon_1),$$

where $\kappa = 2\pi/\lambda$, and $n = \kappa V$. From this we see that, as in accordance with the suppositions already made,

$$\frac{\delta n_1}{\delta \kappa_1} = \frac{\delta n_2}{\delta \kappa_2} = \dots = \frac{dn}{d\kappa},$$

the deviation from the simple harmonic type travel with velocity $d n / d \kappa$, and not with velocity n / κ , that is with velocity $d (\kappa V) / d \kappa$, and not with velocity V .

I now pass on to the theory of Foucault's experiment. If D be the distance between the fixed and moving mirrors, ω the angular velocity of the latter, then the angle through which the mirror turns in the time occupied by the wave in making the double journey is $2 D \omega / V$, and the angular deflection θ , which is the immediate subject of observation, is according to the usual view—

$$\theta = \frac{4 D \omega}{V}.$$

Now it is here assumed that the deflection is due merely to the change of position of the mirror between the two reflections, and that the wave returns to the mirror with its front parallel to the position occupied immediately after the first reflection, as would be the case if the mirror were at rest. But if V be a function of λ , this assumption is not true. Besides the deflection above considered, there is another depending upon the fact that the wave front rotates in the air between the two reflections. The rotation is a consequence of the inclination to one another of successive wave fronts, which involves a variation of wave-length and therefore of velocity at points situated on the same wave-front in a line perpendicular to the axis of rotation. Denoting distances measured along this line by x , we have for the angular velocity of the wave's rotation—

$$\omega' = \frac{dV}{dx} = \frac{dV}{d\lambda} \frac{d\lambda}{dx},$$

in which $d\lambda/dx$, representing the angle between successive wave-fronts of similar phase, is equal to $2\pi\lambda/V$. Accordingly—

$$\omega' = 2\pi \frac{d \log V}{d \log \lambda},$$

and the actually observed rotation is—

$$\theta = \frac{4 D \omega}{V} \left(1 - \frac{d \log V}{d \log \lambda} \right).$$

The result of a calculation which leaves the aerial rotation out of account is therefore not V , but—

$$\frac{V}{1 - \frac{d \log V}{d \log \lambda}}$$

Now

$$U = \frac{d(\kappa V)}{d\kappa} = V \left(1 + \frac{d \log V}{d \log \kappa} \right) = V \left(1 - \frac{d \log V}{d \log \lambda} \right);$$

so that the result of the experiment is V^2/U , and not as previously stated the group velocity U itself. The error arose from a mistake as to the direction of the effect of ω' .

The force of the arguments which I founded upon these considerations is increased rather than diminished by the correction, and with Mr. Michelson's evidence on the same side of the question almost excludes any appreciable variation of V . It should be noticed that by the combination of the two methods of the toothed wheel and of the revolving mirror we have the means of determining both V and U , and the results of Cornu and Michelson appear to prove, independently of astronomical observation, that there is no sensible difference between them.

Indeed by a slightly varied arrangement it would seem possible to determine V directly from Foucault's experiment. If a convex lens were so interposed at the distant station that the fixed mirror occupied its focus, the sides of short and long wave-length would be in exchanged, and thus the rotation acquired during the outward journey would be neutralised during the return.

RAYLEIGH

The Struggle of Parts in the Organism

I AM very glad to learn that Mr. Romanes fully accepts as "well-known and unquestionable" the definition of the term *law of nature* which I propounded as expressing its true scientific sense; but I would suggest to him, as to other writers who are accustomed to speak of such laws as "governing" phenomena,¹ whether the use of such "metaphorical" language is not objectionable, as tending to keep up in the unscientific mind the notion of the "coercive" and "self-sufficient" agency of natural laws. I am glad also to be able to express my entire accordance with Mr. Romanes in regard to the inferiority of the teleological argument based on *special instances* of adaptation of means to ends, to that which is based on the *general order* which we designate by the term law. For I maintained this view even in that remote pre-Darwinian age in which my scientific life commenced, urging to the best of my young ability, forty-three years ago,² that the principles admirably laid down by Whewell in regard to physical inquiry, viz. that final causes should be excluded, because "we are not to assume that we know the objects of the Creator's design, and put the assumed purpose in the place of a physical cause," and that "the notion of design and end is transferred by the researches of science from the

¹ I continually meet with this phrase in the pages of *NATURE*.

² See *British and Foreign Medical Review* for April, 1838: "Physiology an Inductive Science."

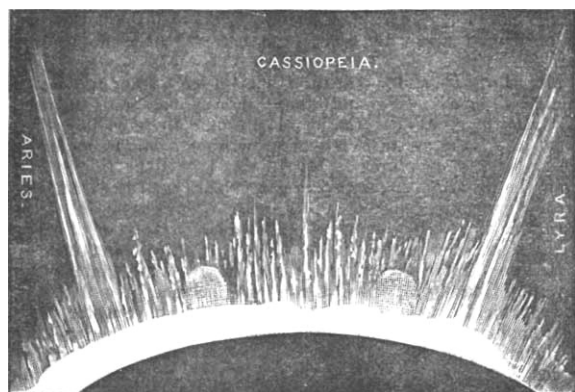
region of facts to that of laws," are no less applicable to physiology than to physics; although Whewell himself (in his "History of the Inductive Sciences") had maintained the contrary. The full acceptance of the doctrine of evolution as our highest expression of the order of creation seems to me to lead to a much nobler conception of the Intelligent Cause of that order than any accumulation of such individual adaptations as might be made by the "mechanic-god" of Paley.

WILLIAM B. CARPENTER

56, Regent's Park Road, N.W., November 14

The Aurora and its Spectrum

YOU have already illustrated in NATURE the aurora and electric storm of January 31. Though somewhat tardily, another drawing with a description of this fine display has found its way into my hands, of sufficient interest, I think, to be added to those already published. It comes from Mr. C. I. Prince, the well-known meteorologist of Crowborough Beacon, Tunbridge Wells, who says, "I inclose a photo of my sketch on a slate.¹ I had a splendid sight of the aurora at about 9h. 15m. It soon became very hazy, but I saw it again at 11h. 15m., when I made the sketch (see drawing). The arch was exceedingly well defined, and extended about 80° along the horizon. At 11h. 20m. some more brilliant streamers shot up along the whole convexity of the arch, and the two patches of light became very tremulous, almost shifting a little from right to left; but I particularly noticed that they did not vibrate *simultaneously*, i.e. if one indicated motion the other was quiescent *until* the first had ceased to show excitement, and this action was alternating for nearly an hour. At 11h. 40m. the arch had much contracted, and by midnight had nearly faded away. The whole phenomenon was



free from colour. I noticed a few small meteors. The night was quite calm; wind south-west."

This account seems to me interesting in connection with an observation made by my friend Dr. Vogel of Potsdam, that during the aurora of April 9, 1871, certain lines in the spectrum alternated in intensity with the character of the discharge, some brightening as others faded.

Mr. Prince does not mention any spectroscopic observations, and while noting this will you permit me to make a few remarks on the "spectrum of the aurora," an old hobby of mine. I notice that Dr. Spottiswoode, P.R.S., in his lecture delivered before the British Association at York on September 5 last (NATURE, vol. xxiv, pp. 572, 3, "On the Electric Discharge, its Forms and Functions,") has referred to the aurora in connection with experiments showing that the discharge in rarefied gases differs from that at higher pressures, and that the difference corresponds to that observed between the flickering play of the aurora and the crashing spark of the lightning-flash. After then referring to the questions of height and colour of the auroral discharge, Prof. Stokes' theoretic view of the connection of earth-currents, disturbances of the magnetic needle, and solar radiation is dwelt upon. In NATURE, vol. xxiv, pp. 613-18 (lecture by Prof. Stokes, Sec.R.S., in the South Kensington Theatre, on "Solar Physics"), this theoretic view is set forth,

¹ I commend this method of sketching to other observers, the slate pencil showing white on the dark slate being readily photographed with good effect, displaying the aurora light on a dark ground.—J. R. C.

and the aurora is described as a flash of lightning passing through the higher regions of the atmosphere where the air is rarefied. There are, I think, some objections to this theory founded upon certain circumstances of the aurora itself, such, for instance, as the well-authenticated cases of auroræ seen close upon the surface of the earth.

The passage, however, in Prof. Stokes' lecture which particularly struck me is this: "But what of the auroræ? It has long been recognised that the aurora is an electrical phenomenon. It has been supposed to be imitated, *and there can be no doubt that the supposition is a correct one* [the italics are mine] by sending an ordinary electric discharge through a highly exhausted tube."

Now it may be true that the aurora is thus imitated so far as external appearance is concerned, and it has long been a favourite idea that this imitation in some way extended to the aurora's actual composition; but what does that Ithuriel spear, the spectrocope, say upon that point—a point which gains the more importance from the fact that such an instrument is mentioned by Prof. Stokes (p. 614) as the true touchstone for the aurora? It says positively that Prof. Piazzzi Smyth's citron line, the one true test of the aurora, has never yet been seen in any electric discharge whatever which we have yet produced, whether in air at ordinary pressure, or rarefied; that the red line (its companion in some auroræ) is equally noticeable for its absence therefrom; and that of the remaining faint and less marked lines one or two only have with doubt and uncertainty been by some referred to the air spectrum as excited by the electric spark or glow. The late Prof. Angström endeavoured to place some of these fainter lines in accord with the spectrum bands of the violet role in Geissler air-tubes, but the comparison failed on critical examination. Prof. Vogel has also considered the aurora might probably be an air-spectrum modified by conditions of temperature and pressure. The Professor's actual line comparisons, however, quite failed as to the citron and red lines, and could hardly be called a success as to the fainter ones. In fact any analogy between the aurora spectrum and the spectrum of the electric discharge in air is all but hypothetical, and the aurora still maintains that mysterious quality which distinguishes it from electric discharges of all sorts, and indeed everything else, viz. its peculiar spectrum.

I therefore again plead the necessity for spectrum observations in connection with the aurora, a point from which Prof. Stokes' lecture, I am afraid, owing to its absence of any remarks on the subject (save that before referred to) is somewhat calculated to draw them away. It is certainly possible that some special gas may exist in the upper regions of the atmosphere here giving rise to the citron, perhaps the red, lines; but then (as Prof. Smyth remarks) if so, why, being an emission spectrum in the aurora, does it not, according to the theory of exchanges, appear as an absorption spectrum or dark lines in the solar spectrum? Even, too, could this be shown, it would still remain an unexplained fact that such a gas has hitherto failed to be recognised in any other body, celestial or terrestrial.

To sum the matter up, the electric discharges in vacuum-tubes, as tested by Prof. Stokes' prism and slit, no more represent the aurora than did the cirrus cloud illuminated by the light of the moon, mentioned by him, which also simulated it.

Upon reliable authority the spectrum of lightning may be considered that ordinarily given by a spark in air; but when we come to rarefied discharges and the aurora, the same comparison does not hold good; and where the electrician has to leave the matter, the spectroscopist has yet to take it up.

To aid in solving the aurora's mystery I would invite all spectroscopists, armed with suitable instruments, persistently to aim at accurate micrometer readings of the aurora spectrum. The approximate places of the lines are pretty well established, but their actual wave-length positions are much wanted, for so only may we hope to master one of the remaining riddles of science.

J. RAND CAPRON

Guildown, November 1

Arctic Research

"PROGRESS of Arctic Research since the Foundation of the British Association," by C. R. Markham, C.B., F.R.S. Such is the title of a very able and instructive paper read before the Geographical Section of the British Association at York on September 6 last, and published *in extenso* in the November number of the *Proceedings* of the Royal Geographical Society. The casual reader of this history may suppose it to be a fair and correct record of half a century of Arctic exploration, and that